Predicting the Effectiveness of Proposed CALFED Actions to recover Salmonids

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0272: Predicting the Effectiveness of Proposed CALFED Actions to recover Salmonids

Final Panel Rating
`inadequate`

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Main thrusts of this proposal are to annually upgrade life-cycle models of winter and spring Chinook already developed by the PIs, to develop such models for fall Chinook and steelhead, and to develop linkages with models of broader ecological processes. The second item in Objective 1 (p. 2) is "Can we gain efficient access to data useful for designing and running the models?" If the answer is "no," then most of the work proposed ($2.5 million) is of questionable feasibility and benefit. This question should be answered as a prerequisite to funding. At least two reviewers feel that the information needed for such a complex modeling effort is not currently available, and that implementation of such a model is not warranted at this time. The length of the Literature Cited (three unpublished reports) does not imply strong familiarity with the literature and what information it contains. On p. 5, it is stated that various agencies "have been encouraged by" accomplishments on a previous model. Letters of support to this effect would be very helpful. Moreover, much of Task 1.1 (p. 5) seems focused on creating a need for the modeling product, rather than responding to a need. The source of "annual monitoring data" used to calibrate the model (p. 6) is never stated, and may not be available as noted above. Although the proposed work (costing over $2.5 million) depends almost entirely on information gathered from

existing literature, the Literature Cited consists of only three unpublished reports. The reviewer has no way of evaluating past unpublished work by the PIs on which the proposed efforts are based, especially the previous review of existing models, and the previously developed model for winter-run Chinook. As this proposal involves little investigative science, but rather application of existing information to management, stronger evidence of familiarity with the literature and the quality of past efforts would be helpful. One reviewer notes that two extensive evaluations of the status of models on this topic for the Sacramento River are available, but are not cited by the PIs. Claimed linkages to models for other species and ecosystem processes are very fuzzy (p. 11-12). It's unclear if the authors have investigated whether models for the processes they list actually exist. Given all the assumptions and uncertainties in algorithms, I suspect that qualitative evaluations of cost-benefit will be as accurate and valuable as output from the proposed models, and that decisions would not be made according to model output. Given that the Literature Cited consists only of three unpublished reports, the focus of the authors on publication in peer-reviewed journals is questionable (only two are proposed from a $2.5 million project).

Additional Comments:

The many undefined acronyms, which are not common to other CALFED proposals, make this proposal very difficult to follow. The budget is excessive, given the work described.

Main thrusts of this proposal are to annually upgrade life-cycle models of winter and spring Chinook already developed by the PIs, to develop such models for fall Chinook and steelhead, and to develop linkages with models of broader ecological processes. The second item in Objective 1 (p. 2) is "Can we gain efficient access to data useful for designing and running the models?" If the answer is "no," then most of the work proposed ($2.5 million) is of questionable feasibility and benefit. This question should be answered as a

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Predicting the Effectiveness of Proposed CALFED Actions to Recover Salmonids

The panel found that

1.The researchers will probably not be able to access sufficient data to parameterize their model. Those data are apparently not available.

2.The proponents showed inadequate knowledge of the literature.

3.The need for the model is not clear. It was insufficiently justified in the proposal. The proponents plan to link their model to other models, but it is not clear that these models are available.

4.Budget was considered very high and was insufficiently justified.

Rating: inadequate

Technical Review #1

proposal title: Predicting the Effectiveness of Proposed CALFED Actions to recover Salmonids

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals, objectives and hypotheses are clearly stated, but very ambitious.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	It is an interesting proposal, but I would guess that existing knowledge is lacking in many respects as far as what is required to do the project properly. I suspect that full-scale implementation may not be warranted at this point in time.
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	

	The approach is interesting and likely to add to the base of knowledge. The information is likely to be useful for decision makers, but lack of knowledge about cause and effect relationships in the system should lead decision makers to use the results from the project with caution until they are demonstrated to match reality reasonably well.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach is well documented and technically feasible in terms of the outputs. The likelihood of success depends on what 'success' means. The project is likely to yield useful tools but in three years I would guess that the outputs from models will still require further study and refinement before they can be relied upon.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Monitoring is not really an important part of the project.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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Comments	Some products of value are likely from the project. If nothing else, areas in need of further research would be identified in terms of the needs for modeling.
Rating	good

Additional Comments

Comments	The use of a simulation modeling approach to assess alternative management actions is obviously useful. However, this does require the system being considered to be well understood so the the model corresponds reasonably well to reality. It is not clear to me that this is the case at present. Furthermore, aspects of the model don't just have to be agreed to by a consensus of experts. They also need to be more or less correct for a model to give useful results.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The track records of the authors seems good based on their past performance. The team seems well qualified with the necessary support for the project.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is very large. I have not checked the details of the costings, which I assume CALFED would do before the project is funded. I feel myself that a less expensive smaller start to the project is easier to justify at this point in time.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	My main concern with this project is that it seems to be to be too optimistic about what is known about the operation of the system being considered. Nevertheless, if it is considered that something like the proposed model is very much needed then it would be better to get started as soon as possible.
Rating	good

Technical Review #2

proposal title: Predicting the Effectiveness of Proposed CALFED Actions to recover Salmonids

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The first goal of the project, to "Develop Models" may be an objective for a consultancy, but not for CALFED. Models are not an end to themselves but are only useful if they can make somewhat credible predictions or if they highlight data gaps and uncertainties. There is no way the proposed model will be able to make even remotely credible predictions. Identification of uncertainties and data gaps has already been clearly articulated in past modelling efforts (e.g. SALMOD) or in previous IMF efforts (e.g. p. 48 Cramer et al. 2003). This information should be used to guide CALFED research and monitoring, but the proposed modelling effort will likely add little to the already large list of uncertainties identified by previous efforts.
Rating	poor

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	This project is poorly justified. As I will discuss in more detail under "Approaches" there are a huge number of uncertainties and structural assumptions in the

	model that are not based on data. In many cases the model consists of borrowed assumptions/relationships from previously unvalidated modelling efforts or rates from hatchery fish applied to wild fish in a natural setting, etc. The model will not be able to make credible predictions. The authors seem unaware of this as evidenced by their response to a critical reviewer in Cramer and Gaigneault (2004): "The IMF is useful in predicting trends or evaluating the relative difference of population metrics based on different scenarios of input parameters." I saw no evidence of the model's ability to predict relative performance and I think this statement reflects the modelling inexperience of the proponents (as justified below). If the model can't make credible predictions, or even get the direction of the prediction correct, why bother to use it in a decision analysis to assess the efficacy of proposed actions. In principle the idea of a decision analysis is good, but one needs the necessary tools before proceeding. In this case the model is currently not available and all the programming, project reports, workshops, glossy model descriptions, web sites, or fancy graphical user interfaces will not change this fact. We simply don't have the scientific understanding.
Rating	poor

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	

There are so many flaws in the overall modelling approach it is hard to know where to begin. I describe only a limited number of the problems to clearly point out fatal flaws of the project.

Model Validation

There proponents are confused, or being directly misleading, on the topic of model validation. The ability to match the historic trend in escapement with a model consisting of hundreds of parameters is not a test or validation of the model's predictive ability. This was clearly pointed out by a reviewer in Cramer and Gaigneault (2004):

(P. 14, comment 17): "The hindcast simulation appears to simulate the decline and recent recover of winter-run population. However, there are many ways to model a similar pattern, and an apparent fit between observed and predicted data does not necessarily indicate that the model has incorporated the appropriate variables in the correct manner."

They proponents claim to have responded to this in comment (3) but their response relates only to whether or not particular processes should be modeled (most of which can't because of insufficient data). They do not respond to the fundamental issue of confounding in this type of model. There are so many parameters and so little independent data that a huge range of parameter combinations can be used to fit the data equally well. However, these parameter ranges can result in very different predictions about the RELATIVE benefits of various restoration strategies WHICH IS THE ULTIMATE OBJECTIVE OF THE MODEL (game over).

I have my doubts about whether the lead modelers understand this issue and the relevance it has to their project. There confusion is apparent in subtasks 3.3.3 where they propose to calibrate the model to the

historic data and then use this to demonstrate the model's fit to observed data! Bottom-line is that we shouldn't have any confidence in even the direction of the response of the predictions given our existing knowledge. There is therefore no point in going through with a decision analysis since the probability of different outcomes is virtually uniform.

Flaws in Model Structure and Parameter Assumptions

The proponents do not present the model structure very well. Whether this was purposeful as a way of masking the major uncertainties, or just due to their inexperience in describing models, is uncertain, but either one is a problem. Any competent modeler should be able to write down the basic equations and assumptions of the proposed model. This is not done in the proposal but is done in a verbal/undergraduate way in Cramer et al. (2003).

Here is a very incomplete list of flaws in model structure:

1. Use of WUA to estimate carrying capacity. Never been shown with data and there is a huge debate in the literature on this which is not even referenced. If they are wrong on this one then they will be wrong on the fish population-flow relationship, which is a big part of the policy-relevant predictions.

2. Very bizarre method used to deal with density-dependence:

"Before subjecting estimated fry production to fixed survival rates used in the JPE model, estimates of fry are compared for a density-dependent pathway (through the Ricker function) to that through a density independent pathway (fry/egg). The simulation proceeds with the least number of fry predicted by these two pathways.

Presumably the real world is a bit more continuous then this! I don't think the proponents know how to incorporate density dependence in the model based on this statement.

3. There are a huge number of additive mortality rates and juvenile maturation schedule parameters that largely have no empirical basis and will all be very confounded with each other.

4. Assumption that either spawning or rearing is the limiting factor. Which is it and how will this be determined? Could make a big difference re. relative responses to key policies.

5. The authors are aware of the many uncertainties in their model as (see p. 48 of Cramer et al. 2003). It amazes me that they want to use it to pursue a decision analysis and develop a total of 4 of these models with the same basic flaws.

6. Develop species interaction functions. No plan here, just go to the literature (which they should already know) and realize that you are going to have to make a bunch of things up like you did for most of the single-species models!

Incorporation of Model Uncertainty

The proponents claim that:

"Stochasticity will be added to the model by incorporating observed levels of variation in the parameters and inputs for key driving functions" (Task 2.3)."

Where exactly is the data to develop the sampling distributions for these key parameters? In many cases the parameters are made-up, based on laboratory study, or inferred from hatchery fish. There is no data to develop credible sampling distributions, let alone

	account for covariation among parameters or uncertainties in model structure. In short there is no way the proponents will be able to do a credible-job of characterizing the huge uncertainty in model predictions. They will produce probabilistic results that could lull naïve participants into the false hope that the uncertainty has been well characterized.
Rating	poor

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	See comments on justification and approach. It is not feasible to develop a model that makes remotely credible predictions to the array of management actions being considered.
Rating	poor

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Not applicable.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	See comments on approach. The only remotely useful product coming out of this project will be yet another list of data gaps and key uncertainties. But since we

	already have lots of those from past modelling efforts it would be better to get out there and start resolving them, rather than invest in developing another such list.
Rating	poor

Additional Comments

Comments	references cited Cramer, S.P., Daigneault, M., and M. Teply. 2003. Conceptual Framework for an integrated life cycle model of winter-run chinook salmon in the Sacramento River (http://www.spcramer.com/imf.htm). Cramer and Daigneault, 2004 (June). Responses to interagency project work team comments on the integrated modeling framework for winter-run Chinook (http://www.spcramer.com/IMF/response%20to%20PWT%20comments.pdf)

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Based on the proposal and the documents on the Cramer and Associates website the proponents are not capable modelers. They demonstrate a fundamental misunderstanding on what model validation is and the utility of models in resource management. Many of their approaches to modelling difficult situations would be corrected in a 1st yr university modelling class. As one example of this experience beyond those mentioned above, consider their plan for Task 2.2: "Convert models to a programming language that accommodates the large calculation and data storage demands." The modelers need do consult with other

modelers and software designers before deciding what language to program their model or interface in? Clearly they haven't developed that many models if they can't figure this one out on their own. That's a tough one to swallow when these modelers are charging $1,000/day and asking for $2.5 million! They should have this easy-stuff dialed, and they clearly don't.

The proposal is quite misleading in terms of the acceptance of the winter-run model in the scientific community:

"Our likelihood of success is indicated by positive responses of CALFED management and NOAA Fisheries to recent briefings on the present winter run model predictions of fish abundance …"

Contrast this with Comment 24 on page 17 of Cramer and Daigneault (2004):

"The model in its current form uses information that has been available and used for years. Much of this information has severe limitations, usually associated with system complexity, natural variation…. We are concerned that the spreadsheet might give some the impression that we have more confidence in these calculations and the data used to make then we really do. Also, at this point, we are not convinced that the model reliably and accurately predicts and interprets the effects of different scenarios on the winter-run populations."

I guess this reviewer doesn't work for CALFED or NOAA! The document that contained these comments was published in June 2004, just a few months before the proposal was written. The proponents were therefore deliberately misleading in their presentation of how accepted their approach is. This is plain dishonest.

Other reviewers who disagree with my comments should take a look at Cramer and Daigneault (2004) for many

	other critical comments on the proposed model. Most of these problems are not fixable given our understanding.
Rating	fair

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	$2.5 million could go a long ways to resolving some of the key data gaps in CALFED rather than on writing code for a model that will make predictions of no credibility, and all the documentation, web sites, workshops, and other busy work that this project entails. The charge-out rates, which are often $1,000/day, are very high, especially considering the lack of substantive modelling experience.
Rating	poor

Overall

Provide a brief explanation of your summary rating.

Comments	It wouldn't surprise me if this project was supported by managers that do not have sufficient technical background to recognize its fundamental flaws. Integration of current understanding into a predictive model to evaluate management actions is a very desirable product. However, our knowledge of habitat-survival-growth-movement interactions at a system-wide scale is not sufficient to support the development of such a model at this time. CALFED needs to invest in well-thought out and coordinated field studies, coupled with informative adaptive management experiments, to resolve these uncertainties. Investment in this type of modelling effort, which is

	heavy on presentation and glossy-models, and light on substance (that is credible predictive ability), has no benefit to CALFED. Models can be useful heuristic tools to identify key uncertainties, but there are already pages of these from past modelling efforts.
Rating	poor

Technical Review #3

proposal title: Predicting the Effectiveness of Proposed CALFED Actions to recover Salmonids

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

	Comments	The goals and objectives are clearly stated in the proposal. The idea is timely and I believe important.
	Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	The is somewhat justified related to existing knowledge. Important steps are identified such as quantifying uncertainty, however objective 4, making ecosystem linkages, perhaps the most important step is not well justified. more information is needed on how specific linkages are going to be made to restoration actions in order to go beyond the typcial life cycle models. Much of this information may currently no existing, so scenarios will have to be developed with differing assumptions.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	`The approach seems appropriate, feasible, and the results should add to our knowledge base. It is unclear how the information will be used by descion makers at this point, although this may not be up to the proponents of the proposal.`
Rating	`good`

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	`The approach is well-document, the likelihood of success seems high based on past performance, and most of the objectives are within the authors grasp. Again linking the restoration response to fish populations seems the weakest link.`
Rating	`good`

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	`monitoring connected to this project would be a good idea`
Rating	`fair`

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	yes the products should be of value. this should be a contribution
Rating	very good

Additional Comments

Comments	

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	project team seems well qualified
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems somewhat large for the tasks, however i am not familiar with consulting costs.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	Overall i think this is a good project. I would say that given the effort involved most should go into

	task 4, which seems to be the crux of the project.
Rating	fair